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EFFECT OF MARRIAGE ON LIFE.*

IN the year 1867 a statement was made by Dr. Stark, Registrar-General for Scotland, which attracted a good deal of notice. He announced, as the result of his investigations into the relative death-rates of married and unmarried men, that the mortality is very much greater among the latter than among the former. Since then several years' statistics have been published by Drs. Stark, Drysdale, and others, in this and other countries, and they appear to corroborate the doctrine that marriage may be regarded as a sort of life-insurance. It is not with any desire to invalidate the truth of this doctrine that we propose here to point out the great uncertainty of such statistics as these. There are enough arguments in favour of matrimony without introducing false ones. An old proverb tells us of the advisability that no cask should rest on an alien basis, and this is especially the case where the basis of our figurative cask is already wide enough to secure stability. The advocates of marriage will therefore, we trust, look upon us rather as an ally than as a foe if we exhibit, as we think we shall be able to do, the unsubstantial nature of the argument based on such statistics as we have referred to above.

Let us in the first place see what the evidence is on which the argument is founded. A single case will suffice. Take the earliest by combining the results of two years' observations. Dr. Stark thus compares the mortality per thousand of married and unmarried men :—

Ages.	Husbands and Widowers.	Unmarried.
20 to 25	6·26	12·31
25 „ 30	8·23	14·94
30 „ 35	8·65	15·94
35 „ 40	11·67	16·02
40 „ 45	14·07	18·35
45 „ 50	17·04	21·18
50 „ 55	19·54	26·34
55 „ 60	26·14	28·54
60 „ 65	35·63	44·54
65 „ 70	52·93	60·21
70 „ 75	81·56	102·17
75 „ 80	117·85	143·94
80 „ 85	173·88	195·40

* This article, excepting a few words relating to its more extended application, and bringing it down to date, was written in the summer of 1867—our readers may probably guess by whom. —SUB-ED.

At first sight, it might seem that nothing could be clearer or more satisfactory than this evidence. We see that between the ages of 20 and 25, the death-rate of the unmarried men is nearly twice that of the married men. After this the ratio gradually diminishes, so that when we come to the quinquennial period between 45 and 50, the ratio, instead of being 2 to 1, is only 21 to 17, but still it is a ratio of *excess;* and so, up to the last recorded period, we find the same evidence in favour of the married men's prospects.

And again, let us take another view of the matter. It is easy to determine the mean age of the married men and of the bachelors at death. We find that *the former age exceeds the latter by fully* 19 *years!*

Here, then, we seem to have the most striking evidence in favour of matrimony as an agent in producing longevity. It would seem almost that all we need fear would be the undue extension of the argument. If one wife does so much to prolong a man's life, what effect, it might be argued, should two, three—nay, a dozen wives, for that matter—not produce? Passing over this view, as a manifest invention of that enemy of social happiness, the confirmed old bachelor, let us seriously inquire what force there really is in the evidence adduced; for the evidence is *not* wholly without force, only it has been asked to bear rather more than it is capable of doing.

There are two most important rules in the application of statistics, for want of attending to which many have fallen into serious error. First, we must assure ourselves that there is nothing in the examples collected which savours of *selection;* and secondly, we must have a *sufficient number* of examples.

As respects the second rule, we do not think there is any reason to complain of the evidence. For although the period over which the results extend is not a very long one, yet the wide range of country included in the registering is fully sufficient to make up for the defect in point of time. In fact, the close accordance observed by Dr. Stark between the results of the first two yearly periods dealt with was quite sufficient to prove that a more extended series of observations was not needed. *Results of this sort only repeat themselves when they are severally founded on a sufficiently wide range of statistical inquiry.*

But, as respects the first rule, we think there is very strong reason for suspecting the evidence before us. We must note, in the first place, that it is one of the most difficult things in the world to free results from "selection" in some form or other. Take the simple instance of tossing up a halfpenny; is the chance perfectly equal that head or tail will turn up? It seems so, but it is not necessarily so. As the halfpenny turns over and over in the air, there may be an irregularity—imperceptible to the sense—due to the unequal distribution of the metal on the two faces. And here we see the importance of the second rule mentioned above. Any irregularity in the figure of the coin will show itself *in a sufficient number of trials, as certainly as by the most accurate measurement and the most careful examination.*

We had lately a remarkable instance in our own experience of the difficulty of removing all trace of selection.

We wished, for a particular purpose, to distribute a number of dots or points, *perfectly at random*, over a square surface. This may seem a simple matter—but we did not find it so. It may be suggested—"Take a handful of grains and throw over the surface at random; then mark the place of each." The fact, however, that the grains *did* form a handful, and were *spread out, will* show itself. Another method may be suggested :—"Prick a number of holes without directing the motion of the

hand by the will." But how are we do this very thing? If we close our eyes we shall naturally make for the central parts of the surface, for fear of missing the surface altogether, and here at once is "selection"; and if we open our eyes it is absolutely impossible not to aim each stroke with *some* object, however much we may persuade ourselves that we are striking quite at random.

The method we finally adopted was this. We divided each side of the square into 100 parts, which we numbered in order, and drawing lines through the points of division we divided the square into 10,000 small squares. We then took a book full of figures (in fact that inviting work, a table of logarithms), and opening at random placed the point of a pencil at random on the page. The figure nearest to the point we marked down, and we took out in this way 4,000 figures. We now took the first four figures —8, 0, 1, 7 say, and did thus with them,—the first two gave the number 80, the next two the number 17, and we accordingly marked a dot on the *eightieth* row of squares, in the *seventeenth* square of the row. Thus we had 1,000 points distributed as we thought quite at random. But on a closer inspection we suspected the influence of selection: and where does the reader suppose we detected it? *In the shapes* of the figures used to represent numbers. In taking the number nearest to the pencil point we had omitted to notice (when the point *seemed* half-way between two figures) that the 1's, the 4's, and the 7's, do not cover *quite* as much space as the other figures. And in fact, when we came to count over our list of numbers we found there was a marked deficiency of these, and a marked excess of 8's, 5's, and 2's. *This excess showed itself* in the arrangement of the dots over the square surface.

We seem to have wandered a long way from our bachelors and married men, but if we have succeeded in showing how subtle an influence selection, conscious or unconscious, is capable of exerting, it will be found that our digression is in reality very much to the point.

It may be asked, "If Dr. Stark took the mortality of the whole population, how can there have been any selection?" We answer by another question, "Is there nothing in the state of bachelorhood itself which affords suspicion of selection?" In answering this question we wish to avoid possible misconception. In dealing with averages, individuals are not to be considered. And therefore, if we say anything of bachelors, as a class, which may seem disparaging, individual bachelors are not to be on that account offended; though, perhaps, many of our readers would not be greatly troubled even if offence were given to a few of the single-minded.

Well, then, it appears to us that if we look on bachelors as a class, we shall see evidence that they are not on a par with married men.

(*To be continued.*)

THE CHEMISTRY OF COOKERY.

By W. Mattieu Williams.

XXVI.

THERE is one more constituent of animal food that demands attention before leaving this part of the subject. This is the fat. We all know that there is a considerable difference between raw fat and cooked fat; but what is the *rationale* of this difference? Is it anything beyond the obvious fusion or semi-fusion of the solid?

These are very natural and simple questions, but in no work on chemistry or technology can I find any answer to them, or even any attempt at an answer. I will therefore do the best I can towards solving the problem in my own way.

All the cookable and eatable fats fall into the class of "fixed oils," so named by chemists to distinguish them from the "volatile oils," otherwise described as "essential oils." The distinction between these two classes is simple enough. The volatile oils (mostly of vegetable origin) may be distilled or simply evaporated away like water or alcohol, and leave no residue. The fixed oils similarly treated are dissociated more or less completely.

Otherwise expressed, the boiling-point of the volatile oils is below their dissociation point. The fixed oils are those which are dissociated at a temperature below their boiling-point.

My object in thus expressing this difference will be understood upon a little reflection. These volatile oils, when heated, being distilled without change are uncookable; while the fixed oils if similarly heated suffer various degrees of change as their temperature is raised, and may be completely decomposed by steady application of heat in a closed vessel without the aid of any other chemical agent than the heat itself. This "destructive distillation" converts them into solid carbon and hydro-carbon gases, similar to those we obtain by the destructive distillation of coal.

If we watch the changes occurring as the heat advances to this complete dissociation point we may observe a gradation of minor or partial dissociation proceeding gradually onward, resembling that which I have already described as occurring when sugar is similarly treated (see No. XIII. of this series).

But in ordinary cooking we do not go so far as to carbonize the fat itself, though we do brown or partially carbonise the membrane which envelopes the fat. What, then, is the nature of this minor dissociation, if such occurs?

Before giving my answer to this question I must explain the chemical constitution of fat. It is a compound of a very weak base with very weak acids. The basic substance is glycerine, the acids (not sour at all, but so named because they combine with bases as the actually sour acids do) are stearic acid, palmitic acid, oleic acid, &c., and bear the general name of fatty acids. They are solid or liquid, according to temperature. When solid they are pearly crystalline substances, when fused they are oily liquids.

To simplify, I will take one of these as a type, and that the one which is the chief constituent of animal fats, viz., stearic acid. I have a lump of it before me. Newly broken through, it might at a distance be mistaken for a piece of Carrara marble. It is granular like the marble, but not so hard, and, when rubbed with the hand, differs from the marble in betraying its origin by a small degree of unctuousness, but can scarcely be described as greasy.

I find by experiment that this may be mixed with glycerine without combination taking place, that when heated with glycerine just to its fusing-point, and the two are agitated together, the combination is by no means complete. Instead of obtaining a soft, smooth fat, I obtain a granular fat, small stearic crystals with glycerine amongst them. It is a *mixture* of stearic acid and glycerine, not a chemical compound; it is stearic acid and glycerine, but not a stearate of glycerine.

A similar separation is what I suppose to occur in the cooking of animal fat. I find that mutton-fat, beef-fat, or other fat when raw, is perfectly smooth, as tested by rubbing a small quantity, free from membrane, between the finger and thumb, or by the still more delicate test of rubbing it between the tip of the tongue and the palate. But dripping, whether of beef, or mutton, or poultry, is

granular, as anybody who has ever eaten bread and dripping knows well enough, and the manufacturers of "butterine," or "bosch," know too well, as the destruction or prevention of this granulation is one of the difficulties of their art.

My theory of the cookery of fat is simply that heat, when continued long enough, or raised sufficiently high, effects an incipient dissociation of the fatty acids from the glycerine, and thus assists the digestive organs by presenting the base and the acids in a condition better fitted (or advanced by one stage) for the new combinations demanded by assimilation. Some physiologists have lately asserted that the fat of our food is not assimilated at all—not laid down again as fat, but is used directly as fuel for the maintenance of animal heat. If this is correct, the advantage of the preliminary dissociation is more decided, for the combustible portion of the fat is its fatty acids; the glycerine is an impediment to combustion, so much so that the modern candle-maker removes it, and thereby greatly improves the combustibility of his candles.

It may be that the glycerine of the fat we eat is assimilated like sugar, while the fatty acids act directly as fuel. This view may reconcile some of the conflicting facts (such as the existence of fat in the carnivora) that stand in the way of the theory of the uses of fat food above referred to, according to which fat is not fattening, and those who would "Bant" should eat fat freely to maintain animal heat, while very abstemious in the consumption of sugar and farinaceous food.

The difference between tallow and dripping is instructive. Their origin is the same; both are melted fats—beef or mutton fats—and both contain the same fatty acids and glycerine, but there is a visible and tangible difference in their molecular condition. Tallow is smooth and homogeneous, dripping decidedly granular.

I attribute this difference to the fact that in rendering tallow, the heat is maintained no longer than is necessary to effect the fusion; while, in the ordinary production of dripping, the fat is exposed in the dripping-pan to a long continuance of heat, besides being highly heated when used in basting. Therefore the dissociation is carried further in the case of the dripping, and the result becomes sensible. I have observed that home-rendered lard, that obtained in English farmhouses, where the "scratchings" (*i.e.*, the membranous parts), are frizzled, is more granular than the lard we now obtain in such abundance from Chicago and other wholesale hog regions. I have not witnessed the lard-rendering at Chicago, but have little doubt that economy of fuel is practised in conducting it, and, therefore, less dissociation would be effected than in the domestic retail process.

Some of the early manufacturers of "bosch" purified their fat by the process recommended and practised by the French Academicians MM. Dubrunfaut and Fua (see *Comptes Rendus*, vol. 71) during the siege of Paris, when they and others read papers on the manufacture of "siege butter" without the aid of the dairy. This consisted in frying the refuse fat from slaughter-houses until the membranous matter and other impurities were carbonised, and thus could be strained away. I wrote about it in 1871, and consequently received some samples of artificial butter thus made in the Midlands. It was pure fat, perfectly wholesome, but although coloured to imitate butter, had the granular character of dripping. Since that time great progress has been made in this branch of industry. I have lately tasted samples of pure "bosch" or "oleomargarine" undistinguishable from churned cream or good butter, though offered for sale at 8½d. per lb. in wholesale packages. In the preparation of this I understand high temperatures are carefully avoided, and by this means the smoothness of pure butter is obtained. I mention this now merely in confirmation of my theory of the *rationale* of fat cookery, but shall return to this subject of "bosch" or "butterine" again, as it has considerable intrinsic interest in reference to our food supplies, and should be better understood than it is.

GAMBLING SUPERSTITIONS.

By RICHARD A. PROCTOR.

(*Continued from page* 12.)

TAKING in their order the gambling superstitions which have been presented above, we have, first of all, to inquire what truth there is in the idea that there are limits beyond which pure chance has no power of introducing peculiar combinations. Let us consider this hypothesis in the light of actual experience. Mr. Steinmetz tells us that, in 1813, a Mr. Ogden wagered 1,000 guineas to one, that "seven" would not be thrown with a pair of dice ten successive times. The wager was accepted (though it was egregiously unfair), and strange to say his opponent threw "seven" *nine times running*. At this point Mr. Ogden offered 470 guineas to be off the bet. But his opponent declined (though the price offered was far beyond the real value of his chance). He cast yet once more, and threw "nine," so that Mr. Ogden won his guinea.

Now, here we have an instance of a most remarkable series of throws, the like of which has never been recorded before or since. Before those throws had been made, it might have been asserted that the throwing of nine successive "sevens" with a pair of dice was a circumstance which chance could never bring about, for experience was as much against such an event as it would seem to be against the turning up of a certain number ten successive times at *roulette*. Yet experience now shows that the thing is possible; and if we are to limit the action of chance, we must assert that the throwing of "seven" *ten* times in succession is an event which will never happen. Yet such a conclusion obviously rests on as unstable a basis as the former, of which experience has disposed. Observe, however, how the two gamblers viewed this very eventuality. Nine successive "sevens" had been thrown; and if there were any truth in the theory that the power of chance was limited, it might have been regarded as all but certain that the next throw would not be a "seven." But a run of bad fortune had so shaken Mr. Ogden's faith in his luck (as well as in the theory of the maturity of the chances) that he was ready to pay 470 guineas (nearly thrice the mathematical value of his opponent's chance) in order to save his endangered thousand; and so confident was his opponent that the run of luck would continue that he declined this very favourable offer. Experience had, in fact, shown both the players, that although "sevens" could not be thrown for ever, yet there was no saying when the throw would change. Both reasoned, probably, that as an eighth throw had followed seven successive throws of "seven" (a wonderful chance), and as a ninth had followed eight successive throws (an unprecedented event), a tenth might well follow the nine (though hitherto no such series of throws had ever been heard of). They were forced as it were by the run of events to reason justly as to the possibility of a tenth throw of "seven"—nay, to exaggerate that possibility into probability; and it appears from the narrative that the strange series of throws quite checked the betting propensities of the bystanders: not one was led to lay the wager (which according to ordinary gambling superstitions would have

been a safe one) that the tenth throw would not give "seven."

We have spoken of the unfairness of the original wager. It may interest our readers to know exactly how much should have been wagered against a single guinea, that ten "sevens" would not be thrown. With a pair of dice there are thirty-six possible throws, and six of these give "seven" as the total. Thus the chance of throwing "seven" is one-sixth, and the chance of throwing "seven" ten times running is obtained by multiplying six into itself ten times, and placing the resulting number under unity, to represent the minute fractional chance required. It will be found that the number thus obtained is 60,466,176, and instead of 1,000 guineas, fairness required that 60,466,175 guineas should have been wagered against one guinea, so enormous are the chances against the occurrence of ten successive throws of "seven." Even against nine successive throws the fair odds would have been 10,077,595 to one, or about 40,000 guineas to a farthing. But when the nine throws of "seven" had been made, the chance of a tenth throw of "seven" was simply one-sixth as at the first trial. If there were any truth in the theory of the "maturity of the chances," the chance of such a throw would of course be greatly diminished. But even taking the mathematical value of the chance, Mr. Ogden need in fairness only have offered a sixth part of 1,001 guineas (the amount of the stakes), or 166 guas. 17s. 6d., to be off his wager. So that his opponent accepted in the first instance an utterly unfair offer, and refused in the second instance a sum exceeding by more than three hundred guineas the real value of his chance.

Closely connected with the theory about the range of possibility in the matter of chance combinations, is the theory of the maturity of the chances,—"the most elementary of the theories on probabilities." It might safely be termed the most mischievous of gambling superstitions.

As an illustration of the application of this theory, we may cite the case of an Englishman, once well known at foreign gambling-tables, who had based a system on a generalisation of this theory. In point of fact the theory asserts that when there has been a run in favour of any particular event, the chances in favour of the event are reduced, and therefore, necessarily, the chances in favour of other events are increased. Now our Englishman watched the play at the *roulette* table for two full hours, carefully noting the numbers which came up during that time. Then, eschewing those numbers which had come up oftenest, he staked his money on those which had come up very seldom or not at all. Here was an infallible system according to "the most elementary of the theories of probability." The tendency of chance-results to right themselves, so that events equally likely in the first instance will occur an equal number of times in the long run, was called into action to enrich our gambler and to ruin the unlucky bankers. Be it noted, in passing, that events do thus right themselves, though this circumstance does not operate quite as the gambler supposed, and cannot be trusted to put a penny into any one's pocket. The system was tried, however, and instead of reasoning respecting its soundness, we may content ourselves with recording the result. On the first day our Englishman won more than seven hundred pounds in a single hour. "His exultation was boundless. He thought he had really discovered the philosopher's stone." Off he went to his bankers, and transmitted the greater portion of his winnings to London. The next day he played and lost fifty pounds; and the following day he achieved the same result, and had to write to town for remittances. In fine, in a week he had lost all the money he won at first, with the exception of fifty pounds, which he reserved to take him home; and being thoroughly convinced of the exceeding fickleness of fortune, he has never staked a sixpence since, and does all in his power to dissuade others from playing."*

(To be continued.)

PLEASANT HOURS WITH THE MICROSCOPE.

By Henry J. Slack, F.G.S., F.R.M.S.

It is probable that the news from France of the vote obtained by Paul Bert against the admission of American pork may excite alarm here, and it is well that the appearance of the little organism whose introduction he fears should be generally known. An interdiction of all pork from the United States is not at all warranted by the state of the case. The *trichina* worm has unfortunately attacked some American pigs, but the great majority are free from the pest, and there is a very simple way of avoiding this, and some similar dangers, by not eating any meat that is insufficiently cooked. The worm cannot resist proper culinary heat, and where it has done much mischief to man there has been the bad habit, as in Germany, of eating raw ham, sausages in a similar state, or acting as pig-killers and butchers sometimes do, holding the operating knives in their mouths.

Slides of muscle containing *trichinæ* can be obtained of the dealers in microscopic objects. I hold one such up to the light, and the naked eye is sufficient to show a number of very small darkish dots. A hand-magnifier of an inch focus leaves little doubt of the nature of the object, which is a small, ovalish cyst about 1-50th of an inch long.† The lowest of Browning's platyscopic lenses, magnifying ten times, enables a little curled object, like a minute twist of thread, to be seen in the clearest of the cysts. Any one acquainted with the creature in its encysted state would at once identify it with that, or a rather higher magnification, and if the least speckiness is noticed in any meat it will be well to make a further examination. If the trichinosis is well developed, the little cysts are extremely numerous, and many of them will be found strengthened and made more conspicuous by a calcareous deposit. Fig. 1, taken from the "Micrographic Dictionary," shows the cyst with the coiled-up worm magnified fifty times linear, and at each end of the cyst is a group of fat cells, which may or may not be found in any particular specimens. Fig. 2 shows the worm out of the cyst and uncoiled, magnified 100 linear. Its mouth is at the extremity of the thin end. In its encysted condition the worm is inactive and sexually immature. If it is swallowed uninjured by certain warm-blooded animals, and gets into their alimentary canals, it develops and produces a numerous offspring, which wander about and finally settle down in voluntary muscles, protected by new cysts. The reader who wishes for detailed information can consult Dr. Cobbold's "Entozoa," and especially the supplement, which contains an account of important experiments carried on at the Royal Veterinary College, under his supervision. Various continental observers contributed to the life history of this creature. To Paget Dr. Cobbold ascribes its discovery, to Owen the first scientific description; and to Leuckart the "first full, complete, and

* From an interesting paper entitled "Le Jeu est fait," in *Chambers's Journal*.

† Cobbold says they average $\frac{1}{75}$ in. in length and $\frac{1}{150}$ in. in breadth.

correct solution of the principal questions relating to the source and mode of genesis of this flesh-worm." The result of experiments in England and abroad has been to show that carnivorous animals, and especially those which subsist on a mixed diet, are most subject to invasion by the parasite. It is possible to rear the worm in herbivorous animals, but in a natural condition it is very unlikely that they will become infected by it, and most authorities will think M. P. Bert's alarm on this score quite unnecessary. Birds do not seem fit hosts for the *trichina*. It can sometimes reach sexual maturity in their intestinal canals, but the development of active wandering specimens does not follow. Experiments with five fowls, one goose, and one crow, carried out by Dr. Cobbold, all yielded negative results. If the worms meet with favourable conditions, their multiplication is enormous. In a case cited by Cobbold, Dr. Thudiccum estimated that a German who died of trichinosis was inhabited by

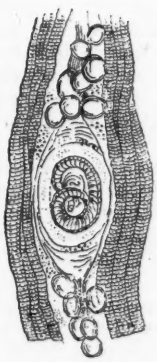


Fig. 1.

Fig. 2.

a population of 40 millions, and Dr. Cobbold thought 100 millions would have been nearer the mark, "as in places the point of a needle could not be turned between the capsules, so closely were they agglomerated." Leuckart reckoned that one ounce of the flesh of a cat he experimented upon harboured three hundred and twenty-five thousand *trichinæ*. These immense worm populations do not take long to form. In less than forty-eight hours the larval worm can attain sexual maturity, and "in six days the female parasites will contain perfectly developed and free embryos in their interior." The young worms do not get into the blood-vessels, but bore their way through the wall of the intestinal canal of their host, and through various tissues till they settle in a voluntary muscle.

Thick joints of infected pork may not in careless cooking get sufficiently heated all through to kill the worms, but in this country the risk is much lessened by the strong dislike most persons feel to pork at all underdone. Even those who can luxuriate upon boiled legs of mutton in a very sanguinary-looking condition, will not touch pork if any redness is visible.

The symptoms of *trichinosis* comprise diarrhœa, fever, and severe muscular pains. If the wandering trichinæ are very numerous, fatal results often occur, and in cases of recovery the patient may still be infected with the parasites in their quiet encysted state.

The professional experts who have to examine infected meat cut thin slices of the muscle, and stretch it carefully on a glass plate. It is then easy to view it with the microscope, and instruments with large stages and special conveniences are made for the purpose.

English pigs reared in farms upon good food are quite safe from the disease; but, without special reference to trichinæ, all pork from animals fed upon garbage, and especially upon butcher's refuse, should be avoided. In America pig-rearing and killing is carried on upon a gigantic scale. As a rule the pigs are remarkably well-fed, as maize and other suitable articles are produced in abundance. Probably, in localities where trichinosis has been common, there has been a great neglect of rational precautions, and the animals have been allowed to swallow filth that should have been kept from them.

DANCING TO DEATH.

WHEN strong and healthy men, inured to hardship by exposure, have to work unduly hard, with insufficient rest and bad food, and under frequent and marked changes of temperature, they sometimes give way. But at this season many delicately-reared girls, who perhaps are not allowed to go out for a walk in unpleasant weather, who have not like their brothers had the healthy training of regular exercise, and who, apart from training, are less fitted by nature to bear cold, heat, and overwork than men, are eager to do work and to bear exposure from which the most stalwart men might with good reason shrink. When the oarsman or the pedestrian, in training for some great race, has done his work for the day, he carefully defends himself against cold or draught. When he has indued dry clothing he takes grateful rest, and each day he has well-regulated sleep. His diet is carefully selected. Everything, in fine, is done to guard, sustain, and increase his vital energies, while the extra work involved in practical training goes on. Is there any kind of work or training much harder than the work done by many young girls in festive seasons? For three or four days each week,—six if they get their wish,—they dance (and talk) almost continuously from nine or so in the evening to two or three or later in the morning. Arms and chest,—too often, also, shoulders and bosom,—are exposed, and suffer alternations of warmth and cold such as a strong, hairy-chested sailor would not care to endure, and certainly would not endure even if he went all day bare-chested and bare-armed. At one moment a fair dancer is perspiring in the warm, moist, and not over-pure air of the ball-room, at another enjoying a refreshing but most dangerous chill in some cool room thoughtfully yet thoughtlessly prepared for the comfort of the dancers. Unwholesome food is supplied to these much-tired girls, at unsuitable and irregular times, and to be consumed under conditions very unfavourable to health. An unwholesome excitement encourages them to exertions far beyond their strength, and keeps them (later) from their rest when rest is most necessary. They grow haggard as the season advances. Worse,—in their opinion—(though in truth it is nature's warning), they cease to be attractive. They grow fretful and irritable—it is the outcry of nature's safety-valve. Well for them if at the end of the festive season they are able to recover a part of their lost energies during a more or less prolonged period of rest and dulness. Not a few sink under the strain. The wonder is that so many survive. One would expect all who have been exposed during several seasons to so severe a trial to show the effects in an after life of weakness and sickness. But although, unfortunately, this is the case with many, there are always some among those who stand many successive seasons of such folly (there

is no other word for it) who may be pointed to as living proofs that there is no real harm in the process—those, to wit, who, being exceptionally strong, have stood the strain best. Just as there are some venerable old ladies who, blessed originally with splendid constitutions, have born unflinchingly and apparently unharmed, the barbarism of tight lacing, and are pointed out by Observers as living proofs that tight lacing does no harm, but is rather beneficial than otherwise (the cause, perhaps, of their fine constitutions!) so there are always many,—so stalwart is our race,—who remaining still strong after years of dissipation, are supposed to prove that there is no harm but rather good in this "sowing of feminine wild oats."

But the wiser among us should teach these foolish ones that they are exposing themselves to a process by which the weaker die, and all—even the strongest—suffer in health and strength.

Well-earned Praise.—The Postmaster-General has issued a circular in which he expresses his high appreciation of the energy and zeal displayed in successfully meeting the unprecedented pressure of Post-office business experienced throughout the United Kingdom during the last days of the past year.

Sense of Direction in Man.—With regard to the question of the means by which animals find their way home from a long distance, a striking account in relation to man will be found in the English translation of the "Expedition to North Siberia," by Von Wrangel. He there describes the wonderful manner in which the natives kept a true course towards a particular spot, while passing for a long distance through hummocky ice, with incessant changes of direction, and with no guide in the heavens or on the frozen sea. He states (but I quote only from memory of many years' standing) that he, an experienced surveyor and using a compass, failed to do that which these savages easily effected. Yet no one will suppose that they possessed any special sense which is quite absent in us. We must bear in mind that neither a compass, nor the north star, nor any other such sign, suffices to guide a man to a particular spot through an intricate country, or through hummocky ice, when many deviations from a straight course are inevitable, unless the deviations are allowed for, or a sort of "dead reckoning" is kept. All men are able to do this in a greater or less degree, and the natives of Siberia apparently to a wonderful extent, though probably in an unconscious manner. This is effected chiefly no doubt by eyesight, but partly, perhaps, by the sense of muscular movement, in the same manner as a man with his eyes blinded can proceed (and some men much better than others) for a short distance in a nearly straight line, or turn at right angles, or back again. The manner in which the sense of direction is sometimes suddenly disarranged in very old and feeble persons, and the feeling of strong distress which, as I know, has been experienced by persons when they have suddenly found out that they have been proceeding in a wholly unexpected and wrong direction, leads to the suspicion that some part of the brain is specialised for the function of direction. Whether animals may not possess the faculty of keeping a dead reckoning of their course in a much more perfect degree than can man; or whether this faculty may not come into play on the commencement of a journey when an animal is shut up in a basket, I will not attempt to discuss, as I have not sufficient data.—Charles Darwin.

HOW TO CHOOSE A TRICYCLE.

By John Browning,

Chairman of the London Tricycle Club.

SCARCELY a day passes without my receiving an application, either personally or by letter, asking me for advice respecting the choice of a tricycle.

In accordance with the request of several of these correspondents I propose to give some general advice on the subject, as just at the present time many persons are doubtless thinking of getting a machine to ride through the approaching season.

I would advise all intending purchasers to give their orders as early as possible. As a general rule, the order is not given until we have had some fine riding weather; then the order is given, and the manufacturer is pressed to execute it instantly. If all orders were given in this way it is evident that the manufacturers would be glutted with orders for two months in the year, and without business for the other ten.

Those tricyclists who already have machines, but wish to try other mounts, would, of course, do well to wait and see the novelties at the three great exhibitions; but those who intend riding for the first time would do best by giving their orders at the dull time, and having a machine carefully made for them exactly suited to their requirements.

Not a week passes without my being asked, "Which is the *best* tricycle?"

There is no such thing. What will suit one part of the country will not suit another, and what will suit one rider well would be almost useless to another. Be sure to deal with a manufacturer or agent of good repute or who has been well recommended to you by some one who has had good experience.

Do not apply to some celebrated rider simply because you have heard of his performances on wheels. Such men are sometimes among the worst judges of a machine. They are so strong that they can drive anything.

When you have decided on giving the order, furnish the following information to the person to whom you give it.

Your height, your weight, your age. Whether you are very powerful, of average strength, or weak. The condition of the roads over which you intend principally to ride—that is, whether they are smooth, fairly good, or rough, and whether they are very hilly, undulating only, or level. All these points ought to be taken into consideration in building a machine to suit you.

It may also be desirable for you to tell him whether you wish to ride fast, that is, at from nine to twelve miles an hour; at a moderate pace, that is, from seven to nine miles an hour, or whether you will be satisfied to get over the ground easily and comfortably, at the rate of from five to seven miles an hour. Do not believe anyone who tells you that it is easy to jog along at a pace of nine miles an hour. A strong and practised rider on a light machine has his work to do to cover nine miles in an hour on a give-and-take road.

You have next to determine what kind of machine you will have.

Be sure you have a double-driver—that is, a machine in which two wheels are driven; as a rule, all single drivers are unsatisfactory. The only exception I can make to this is in the case of the Coventry Rotary; that machine is in a class by itself. It is light and wonderfully sensitive in steering.

If you want the fastest machine you can get, and do not mind giving some little time to practising and running some risk while learning, then have a "Humber." But

I do not consider that this is a good machine for stout, elderly men.

The safest and best all-round machines are the front-steerers, known as the "Rucker," the "Salvo," the "Apollo," and the "Premier." If you wish for a rear-steerer, choose the "Rover" or the "Rucker."

Be sure the machine has a band-brake. Though some of the tyre-brakes are efficient when first made, they are very liable to get out of order. One of the spoons then comes on to the tyre of one of the wheels before the other. If in this condition the brake is applied to check the speed of the machine when descending a hill, one wheel is stopped while the other runs on; as a result of this, if the pace is at all great, the machine swerves and turns over, with the risk of seriously injuring the rider. The band-brake in a double-driver checks both wheels equally—hence its great superiority.

The next point to consider is the size of the wheels. If you want a compact, strong, and yet light machine, let these be as small as you can ride with advantage; the only point to observe being that your heels must clear the axle when you are pedalling.

If for a front steerer, this will be 40 inches for a man 5 ft. 6 in. high, and for each two inches that the rider is below this height the wheels may be one inch less, and for each two inches he is above this height the wheels should be one inch more, so that a rider 6 ft. high may ride a machine with wheels 44 in. diameter, as 43 in. are seldom or never made.

The weight of a roadster machine of this size, for a rider weighing 11 stone, ought not to exceed 80 lb., and a machine with hollow rims, small rubber tyres (some $\frac{1}{2}$-inch in diameter), and laced spokes, may be made not to exceed 65 lb., but such a machine would require using carefully over rough roads.

The spring is a most important part of the machine, and Harrington's Arab Cradle Spring is the best.

Lamplough's long-distance suspension saddle I prefer to any other. Cushioned saddles are not nearly so good.

Ladies should always use a saddle in preference to a seat, and ride as high as they well can, and well over their pedals; they will then find none of the tendency to slip off that they often complain of when riding on a seat, and if the saddle is only two or three inches behind the pedals, they never need when riding show even the tops of their boots.

The handle-rods as well as the seat-rod should be adjustable to suit the rider, and the seat should have an adjustment to and fro as well as up and down. The best contrivance for enabling this to be done is an angle pin, which can be shown in print by a letter L reversed in both directions (⅂). The vertical part of the L moves up and down, and the seat moves to and fro on the horizontal portion.

I must reserve my remarks upon foot-rests until my next article.

INSTINCT.—From the case of neuter instincts, of certain reflex actions, and of movements such as those of the tumbler-pigeon, it seems to me in the highest degree probable that many instincts have originated from modifications or variations in the brain, which we in our ignorance most improperly call spontaneous or accidental; such variations having led, independently of experience and of habit, to changes in pre-existing instincts, or to quite new instincts, and these, proving of service to the species, have been preserved and fixed, being, however, often strengthened or improved by subsequent habit.—CHARLES DARWIN.

ALMANAC LESSONS.

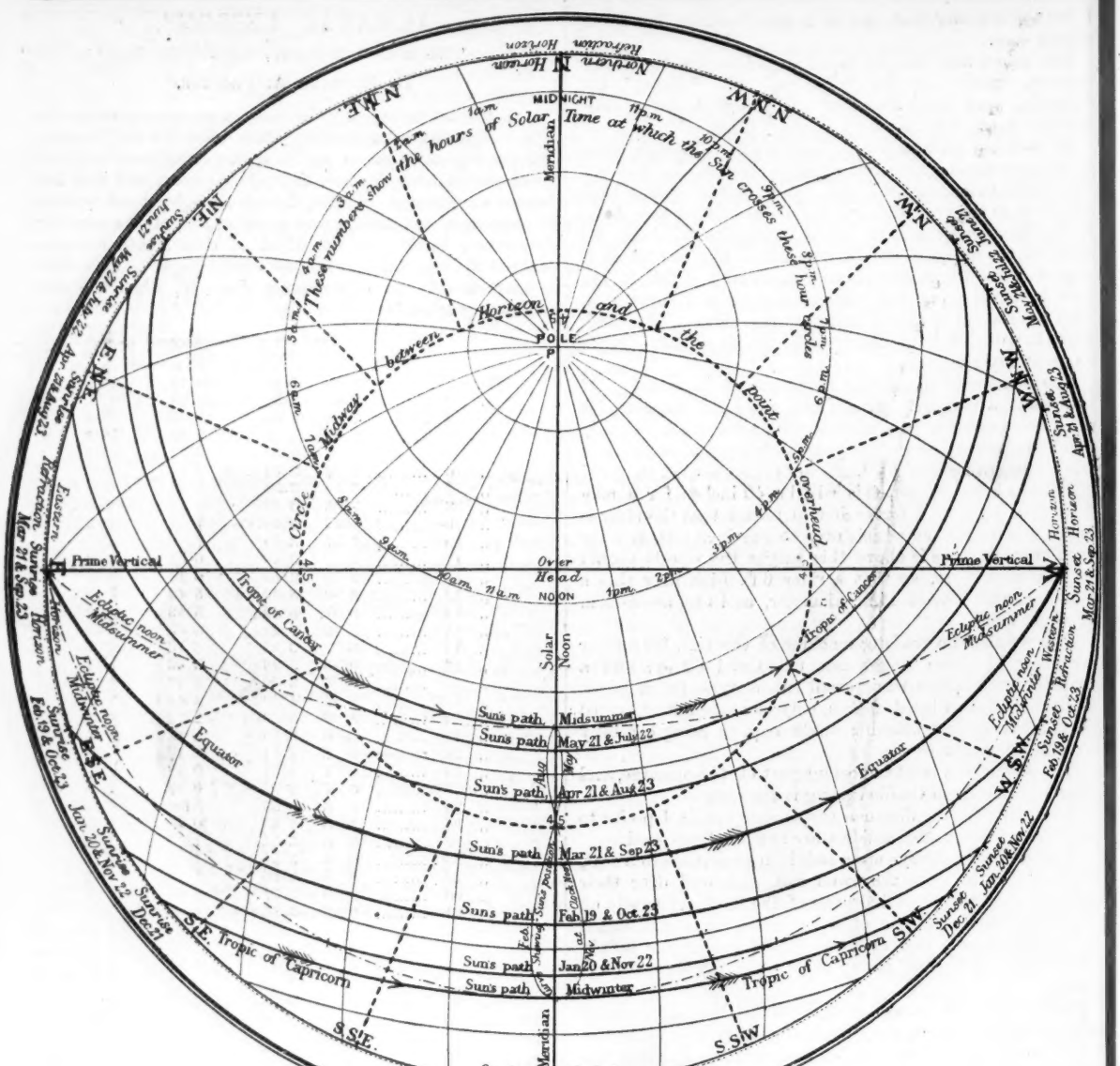
THE SUN'S RISINGS, SETTINGS, &c.

BY RICHARD A. PROCTOR.

AS the astronomical year may most conveniently be regarded as beginning either when the sun is crossing the equator, that is at the spring or autumn equinox, or else when he is farthest from the equator, that is at the winter or summer solstice, I have thought it convenient to reproduce the risings and settings, &c., of the sun for December, 1883. On the 22nd of that month the sun entered the sign Capricornus, or winter began. This date is marked off. In like manner Jan. 20, when the sun enters Aquarius, is marked off.

		The Sun Rises.	The Sun Sets.	Before Clock.	Hourly Variation of Equation of Time.
		H. M.	H. M.	M. S.	S.
Dec.	1	7 46	3 53	10 51	0·94
,,	2	7 47	3 52	10 28	0·97
,,	3	7 48	3 51	10 5	0·99
,,	4	7 50	3 51	9 41	1·02
,,	5	7 51	3 50	9 16	1·04
,,	6	7 52	3 50	8 51	1·06
,,	7	7 54	3 50	8 25	1·08
,,	8	7 55	3 49	7 59	1·10
,,	9	7 56	3 49	7 32	1·12
,,	10	7 57	3 49	7 5	1·14
,,	11	7 58	3 49	6 38	1·15
,,	12	7 59	3 49	6 10	1·17
,,	13	8 0	3 49	5 42	1·18
,,	14	8 1	3 49	5 13	1·19
,,	15	8 2	3 49	4 45	1·21
,,	16	8 2	3 49	4 16	1·22
,,	17	8 3	3 49	3 46	1·22
,,	18	8 4	3 49	3 17	1·23
,,	19	8 5	3 50	2 47	1·24
,,	20	8 6	3 50	2 17	1·24
,,	21	8 6	3 50	1 47	1·25
,,	22	8 7	3 51	1 17	1·25
,,	23	8 7	3 51	0 47	1·25
,,	24	8 7	3 52	0 17	1·25
,,	25	8 8	3 53	After	1·25
,,	26	8 8	3 53	0 42	1·24
,,	27	8 8	3 54	1 12	1·24
,,	28	8 8	3 55	1 42	1·23
,,	29	8 9	3 56	2 11	1·22
,,	30	8 9	3 57	2 40	1·21
,,	31	8 9	3 58	3 9	1·20
Jan.	1	8 9	3 59	3 38	1·19
,,	2	8 8	4 0	4 7	1·18
,,	3	8 8	4 1	4 35	1·16
,,	4	8 8	4 2	5 2	1·14
,,	5	8 8	4 3	5 29	1·12
,,	6	8 7	4 4	5 56	1·10
,,	7	8 7	4 6	6 22	1·08
,,	8	8 7	4 7	6 48	1·06
,,	9	8 6	4 8	7 13	1·04
,,	10	8 5	4 10	7 38	1·02
,,	11	8 5	4 11	8 2	0·99
,,	12	8 4	4 12	8 25	0·97
,,	13	8 4	4 14	8 48	0·94
,,	14	8 3	4 15	9 10	0·91
,,	15	8 2	4 17	9 32	0·89
,,	16	8 1	4 19	9 53	0·86
,,	17	8 0	4 20	10 13	0·83
,,	18	7 59	4 22	10 33	0·80
,,	19	7 58	4 23	10 52	0·77
,,	20	7 57	4 25	11 10	0·74
,,	21	7 56	4 27	11 27	0·71
,,	22	7 55	4 28	11 44	0·68
,,	23	7 54	4 30	12 0	0·65
,,	24	7 53	4 32	12 15	0·61
,,	25	7 51	4 34	12 29	0·58
,,	26	7 50	4 35	12 43	0·55
,,	27	7 49	4 37	12 55	0·52
,,	28	7 47	4 39	13 7	0·48
,,	29	7 46	4 41	13 19	0·45
,,	30	7 44	4 42	13 29	0·41
,,	31	7 43	4 44	13 38	0·38

What appears most striking at first in the numbers here



Illustrating the Sun's Diurnal Course throughout the year. (In this Map equal distances from the centre represent equal distances on the Sky from the point overhead.)

tabulated is that the hour of sunrise grows later up to the end of December, eight or nine days after the winter solstice, when, the sun being at his greatest distance south of the equator, one would expect him to rise latest; while, on the other hand, the hour of sunset gets earlier only until about December 12th. The day indeed grows shorter until December 21-22, but in a seemingly unsymmetrical way, shortening in the morning and lengthening—though rather less—in the evening. It follows from this that true mid-day—that is the time when the sun is due south and at his highest—is changing during this part of December, getting later and later. This is shown by the next column. The sun is before the clock at the beginning of December by nearly 11 minutes; that is the sun comes to the south nearly 11 minutes before clock noon, and of course he rises earlier and sets later than he would if he came to the south at true noon. The day lasts 8 hours 7 minutes at this time, and if solar noon fell at 12 o'clock the sun would rise at 7 hours 56½ minutes, and set at 4 hours 3½ minutes. But both these phenomena take place nearly 11 minutes earlier. Every day until Christmas the sun loses more and more of this advance, until at or about Christmas-day the day is symmetrical, the interval from sunrise to clock noon being equal to the interval from clock noon to sunset. After this solar noon occurs follows clock noon, by an interval gradually increasing all through January and afterwards, as will be seen in due course, till February 11th.

This is partially explained in the accompanying diagram showing the sun's daily path in different parts of the year. The short daily course of the sun at midwinter is shown—marked Tropic of Capricorn. The actual annual path of the sun in the celestial sphere carries him along the ecliptic in a contrary direction from his diurnal course; and of course the more rapidly he crosses the hour circles *that* way the later he must be in being brought by the diurnal revotion to the meridian or due south. Now, not only is he near perihelion in December and January, and therefore moving faster than his average rate along the ecliptic, but also as the diagram shows he is traversing the hour circles where they lie nearer together than on the equator. Hence all this time, he is falling towards the east with more than average rapidity, and the solar day is therefore increasing in length. The portion of the long double loop in the meridian, between the little lines made on the sun's mid-winter path, slightly to the right or west of the meridian, and where the word February is written, (in the eastern part of the lower loop) shows how the sun's position at clock noon passes at this season farther and farther east, or causes him to come later and later up to the meridian.

But this double loop illustrates so much better than any plan I have yet seen what is called the equation of time, that it will be well to have—shortly—a drawing of it on a better scale.

(*To be continued.*)

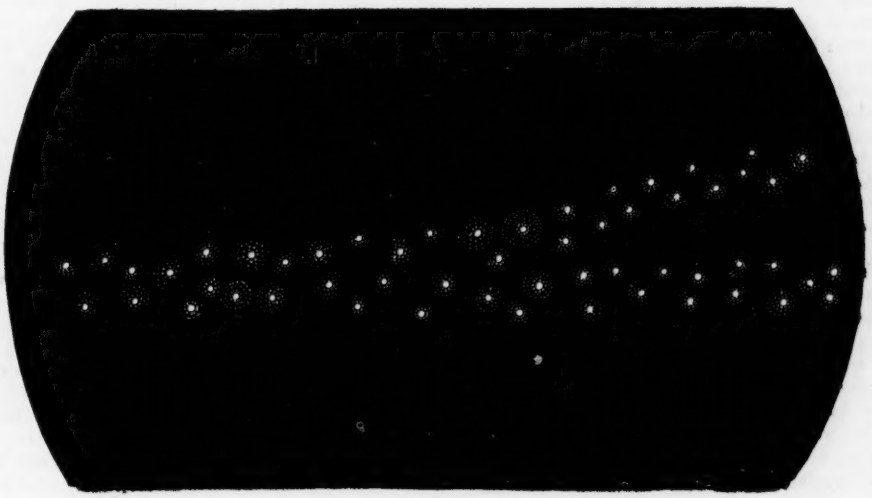


Fig. 3.—Illustrating Lambert's Theory of our Stellar System.

THE UNIVERSE OF SUNS.

By R. A. Proctor.

(*Continued from p.* 10.)

IN passing from Kant's theory to Lambert's, I would invite the reader to note with special care the points in which the two theories differ. It is admitted that Lambert's theory though published five years later than Kant's (or in 1760)* was formed quite independently; but Kant himself remarked in 1763 that "the accordance between his ideas and those of Lambert (*cet homme spirituel*) extends even to the most minute details." It will be found, I conceive, that no such close agreement exists between the two theories.

Lambert's theory consists of the following theses:—

1. The stars are suns, and like our sun they are the centres of planetary systems. Each sun with its dependent worlds forms a *system of the first order*. (It will be observed that this agrees with Kant's first thesis,—which, however, was earlier enunciated by Huyghens.)

2. Our sun belongs to a vast globular cluster of stars, *a system of the second order*, which includes all those stars, spread over all parts of the heavens, which do not belong to the Milky Way.

3. There are many systems of the second order. They are not spread throughout all space, but are all found near a certain principal plane or mean level, and being ranged one behind the other to a great depth, they form by their concourse the Milky Way. This is *a system of the third order*, not globular but flat,—forming, in fact, a disc, whose diameter vastly exceeds its thickness. The fact that this system of the third order consists of different systems of the second order, is shown, says Lambert, by the irregularity of the Milky Way, by the different richness of its various parts, and by its branching figure. (It will be noticed that Lambert here adopts a view resembling a part of Wright's theory on which Kant says nothing; but Lambert's views are much more definite than Wright's, and accord much better with the results of Sir W. Herschel's later labours.)

4. Analogy suggests that there are in the universe many Milky Ways. Perhaps the nebula in Orion is a Milky Way, nearer to us than the rest. Should this be the case, telescopic research will reveal many others, forming together *a system of the fourth order*.

5. Analogy suggests that there are *systems of a fifth order*, and of yet higher orders. But so far as observation is concerned, stellar astronomy includes—1, suns; 2, clusters of suns; 3, the Milky Way; and 4, systems of Milky Ways.

6. The common bond of different systems of the same order, as well as of the successive orders of systems, is gravitation, which produces concentric motions within all the systems.

* More fully however in 1761. Lambert tells us that his theory was formed in 1749, when he was not 21 years old. Kant was born in 1724, four years before Lambert: but survived him 27 years, for Lambert died in 1777, Kant in 1804.

7. As in the solar system we observe that the distances between the several planets exceed incomparably the dimensions of each planet or scheme of planet and satellites, so the distances between sun and sun exceeds enormously the dimensions of the several solar systems.*

8. A similar relation prevails amongst higher orders. The cluster to which the sun belongs contains above a million and a half of stars, forming a spherical cluster, whose diameter exceeds 150 times the distance of Sirius; but this diameter is far less than the distance of the nearest cluster of suns.

Other theses which follow need not here be considered, because they relate to matters already discussed, and are for the most part based on imperfect evidence, and also (as the elder Struve has shown) on incorrect ideas. But Lambert's conceptions respecting central suns are worthy of notice. They are included in the following theses:—

9. It seems clear from the analogy of the solar system, that each of the clusters of suns must be ruled by a central orb. No otherwise, it would appear, can the motions within the cluster have the requisite stability.

10. The mass of such a central sun must be very great. Its luminosity may be faint, or it may even be an opaque body, illuminated by the suns which travel nearest to it. If suns are intended to illuminate dark orbs, we may infer that the central orb is thus illuminated. So that whereas in solar systems the central body illuminates the rest, in systems of suns the central orb receives light from the bodies over which it bears sway.

11. The central orb may one day be detected by the perturbations which it produces within our solar system,—much as the sun produces perturbations in the lunar motions. Or it may have phases, or dark spots, and so be detected by its rotation. On this account the nebula in Orion, which may be the central orb of our cluster of suns (since it lies in the proper direction) should be carefully watched.

12. Having accepted the belief that a central body sways each cluster of suns, we may infer from analogy that a central orb of yet vaster dimensions sways the Milky Way or the system of sun-clusters. And the analogy may be extended to the systems of the fourth, fifth, and higher orders.

I cannot better close the account of Lambert's remarkable speculations than by quoting the remarks in which he describes the wonderful grandeur of the universe of systems as pictured in his theory:—"How far soever we may extend the scale," he says, "we must necessarily stop at last." And *where?* At the centre of centres, at the centre of creation, which I should be inclined to term the capital of the universe, inasmuch as thence originated motion of every kind, and there stands the great wheel in which work the teeth of all the rest. From thence the laws are issued which govern and uphold the universe; or rather, there they resolve themselves into one law of all others the most simple. But who would be competent to measure the space and time which all the globes, all the worlds, all the worlds of worlds, employ in revolving round that immense body—the Throne of Nature and the Footstool of the Divinity! What painter, what poet, what imagination is sufficiently exalted to describe the beauty, the magnificence, the grandeur, of this source of all that is beautiful, great, and magnificent; and from whence order and harmony flow in eternal streams through the whole bounds of the universe!"

(*To be concluded.*)

* Lambert considered that the distance between the nearest fixed star and the sun, exceeds the earth's distance from the sun 500,000 times.

ROVE-BEETLES.

By E. A. Butler, B.A., B.Sc.

THOUGH a very numerous and useful group of insects, but few of the rove-beetles are generally known, and these are not respected as they should be. The largest and most familiar member of the family is called the "Devil's Coach-Horse"—a fanciful name suggested by the repulsive aspect, disgusting odour, and ferocious nature of the creature. It is a dull black, narrow, and elongate monster (Fig. 1), a little over an inch in length, often seen scuttling along a pathway, ever ready to assume the offensive, both as regards attitude and smell. Let it be menaced, say, by the end of a walking-stick, and forthwith it faces round to give battle, opening a formidable pair of sickle-shaped jaws, and bending the hinder part of its body over its back as if to suggest that, like the scorpion, it carries a sting in its tail. Though this is not the case, yet in the tail is lodged what is frequently almost as effective as a sting, a pair of soft, yellowish vesicles, which can be protruded at pleasure, and which exhale a most abominable odour, and the insect is not slow to bring them into requisition. Here, then, we have a being armed at each end, jaws in front, scent-bags behind, so that, attack him which end you will, he is ready for you.

This well-known insect, to which entomologists have given the appropriate name *Ocypus olens*, the "stinking swift-foot," may serve as the type of the group. Rove-beetles, then, are very readily recognised by their narrow and elongate form, and by the shortness of the wing-covers, or elytra, the sheaths which, as in the ladybirds of which we spoke a short time ago, cover the membranous wings. These are so small that, instead of extending over the whole abdomen, as is usually the case with beetles, they cover only the basal part of it, leaving all the rest, to the extent generally of some six or seven segments, entirely exposed. This peculiar arrangement gives the insects a superficial resemblance to earwigs, with which, in fact, the larger species are not unfrequently confounded. The wings of earwigs, however, are altogether different from those of rove-beetles, and, what is of more importance, the developmental history of an earwig is similar to that of a bug, and quite different from that of a beetle; in other words, young earwigs are very similar in form to the adult, but juvenile rove-beetles are grub-like, and wholly unlike their parents until they arrive at their final stage. The extreme shortness of the elytra, which in this group exhibit the minimum of development, has suggested the name Brachelytra (short wing-covers), by which the creatures are known among entomologists. Though the elytra are so small, the wings are generally ample, and, when not in use, have to be very neatly folded and packed away under their horny covers. The insect uses its long, flexible abdomen to assist it in this operation, turning its tail over its back and deftly tucking the gauzy wings into place.

The Brachelytra are, many of them, devourers of all sorts of decaying organic matter. They may be found plentifully in or under the carcases of small quadrupeds, birds, &c.; a keeper's tree, to which "vermin" are nailed, scarecrows, dead rabbits, mice, moles, and so on yield them in plenty. They occur in great abundance in all kinds of animal excrement and in manure-heaps, under heaps of vegetable rubbish, dead leaves, &c.; in fungi, either when fresh or when decaying, but especially the latter; in heaps of rotting seaweed, amongst the filthy accumulations on the floors of fowl-houses, and among the rubbish at the bottom of hay-stacks. Generally, the more rotten the matter, and therefore the more necessary its speedy removal, the more

numerous are the rove-beetles. Some, however, frequent much more attractive localities, such as moss, and even flowers. Fig. 4 represents a species that often swarms in flowers, especially those of the Umbelliferæ. It belongs to a section that, in the shortness and breadth of the body and the greater comparative length of the elytra, departs a good deal from the ordinary Brachelytrous type. Some are found under the bark of trees, some burrow in sandy places, others frequent very damp spots, such as mud-flats and the margins of rivers and ponds, running actively about, sometimes almost in the water itself. One remarkable genus called *Stenus* is specially noteworthy as coming under this last category. There is a very strong family likeness between the 65 British species of this genus, by which they can be easily recognised. They are of a deep-black or leaden-black colour, occasionally with a small yellow spot on each elytron; they have very prominent eyes, a cylindrical thorax, a long cylindrical body, flattened above, and a very hard integument; they have a peculiar impish sort of look, and the appearance of the whole genus is as though they had been most grievously afflicted with small-pox, for the whole surface—head, thorax, elytra, abdomen, and all—is always closely and deeply fitted with minute rounded depressions. One of the largest and most robust of the genus is represented at Fig. 2.

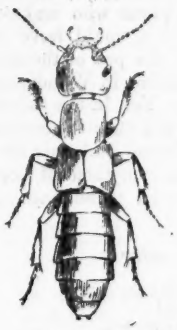


Fig. 1. Ocypus olens. *a*. Nat. size.

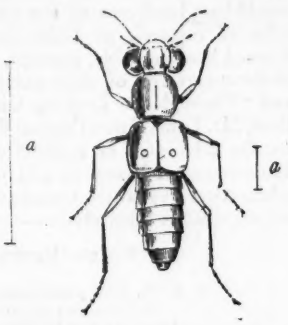


Fig. 2. Stenus bimaculatus. *a*. Nat. size.

One of the most curious habits of the Brachelytra is a kind of parasitism, or rather commensalism, which is exhibited, however, by only a few species. These are always found in ants' nests, where they live on terms of the closest intimacy with the legitimate occupants, and each species is usually associated with only one kind of ant. It is difficult to say what is the exact nature of the bond that leads two such dissimilar insects to unite their fortunes; it has been suggested that the beetles may furnish some secretion capable of being used as food for the ant-grubs; but, however this may be, and whatever other services the Brachelytra may render, it seems probable that they may, at least, act the part of scavengers to these large communities. At any rate, the ants seem greatly attached to them, sometimes even gently carrying them away in their jaws to places of security, when any danger threatens. Fig. 3 represents one of these myrmecophilous beetles; it lives with the well-known Wood Ant, the huge mounds of which are familiar objects in game preserves. It should be remembered that beetles are not the only kinds of creatures that are associated with ants; some ants carefully watch over and maintain communities of aphides, or plant-lice, which yield them the honeydew of which they are fond; and, again, in some ants' nests is found a peculiar white wood-louse, which rejoices in the euphonious appellation of *Platyarthrus Hoffmannseggii*.

Rove-beetles readily take to the wing, and though ordinarily concealed at the roots of grass, under stones, or amongst their favourite food, bright summer sunshine invariably calls them forth from their hiding-places, and then they fly vigorously about seeking pastures new, as well as a suitable nidus for their eggs. Immense numbers of the smaller kinds may thus be found flying about over any recent deposits of horse-dung, or about manure-heaps, or piles of garden-refuse. Not unfrequently an exceptionally fine and warm day in summer entices enormous swarms from their places of concealment, and they invade our streets, appearing sometimes in such numbers on the pathways that the unwary steps of pedestrians leave their mangled carcases dotting the pavement in all directions. When the sunshine departs, those that are left disappear as suddenly as they appeared, retiring into nooks and crannies and obscure places, which none but themselves know so well how to find.

Most of them are extremely active, not only on the wing, but on the ground as well, where their rapid movements frequently baffle the collector who attempts to seize them by darting at them with finger and thumb. When surprised, some rush away with a sharp, jerking motion, stopping ever and anon as if to note the effects of their tactics, but most go headlong forward, with a trotting sort of gait, in the first direction that offers, making the most energetic, if not always the most intelligent, efforts to reach some place of concealment.

Fig. 3. Dinarda Märkeli. *a*. Nat. size.

Fig. 4. Anthobium ophthalmicum. *a*. Nat. size.

Upwards of 700 species of these creatures have been met with in the British Isles, and they vary in size from 1-24th of an inch to a little over an inch in length. By far the greater number, however, range from 1-12th to ¼ inch. It will, of course, be easily understood that as the Brachelytrous type has had, as it were, so many changes rung upon it, and as the insects themselves are in so many instances exceedingly minute, the distinctions between the different species are not unfrequently slight. In fact, it is often extremely difficult to discriminate between closely-allied species, and the assistance of some kind of magnifying power has generally to be evoked; the mere detection of the difference is difficult to any but the practised eye, but when once recognised they are found to be constant and reliable.

The range of coloration is not very great; black is by far the commonest colour; some species are more or less of a metallic green or bronze; others are adorned with bright red; and others again with tints varying from a pale yellowish brown, to a deep blackish brown; but, as a rule, they are not noted for either brilliance or elegance of colouring.

Many species, especially the larger ones, smell badly, as might have been expected, considering the nature of their food. They are, however, an extremely useful group of insects; they compensate for the smallness of their size by

their amazing numbers, and the amount of decaying matter annually caused to disappear from the earth through their efforts must be considerable, though on such a point it is, of course, impossible to obtain anything like reliable statistics. But if it be remembered that during the summer months there are, say in our own country alone, millions upon millions of them continually at work as scavengers wherever any putrescent matter is found, it will readily be believed that they discharge a most useful function in the scheme of nature. In the winter those that have survived the vicissitudes of the season retire into seclusion, and remain in a torpid condition till the genial warmth of the returning spring summons them again to activity, just at the time that the processes of decay are being by the same power accelerated. It must not be supposed that the Brachelytra confine their attacks to carrion and refuse; many of them, especially the larger sorts, are in the highest degree predatory, devouring with avidity even insects of their own kind, and many, no doubt, take equally readily to both living and dead food.

The *Gaceta Industrial* says a concentrated solution of bichromate of potash and glue makes an excellent cement for repairing articles of broken glass. After covering the fractured surfaces with this solution they are brought together and exposed to the action of the sun. This cement resists boiling water.

At Jonkoping, Sweden, is the oldest and largest match factory in the world. It was established 100 years ago, and there are now to be seen specimens of the matches used at the beginning of the present century, consisting of big fagots of wood furnished with a handle and a tip to dip in a bath of sulphur. The wood from which the present kind of matches is made is taken from the adjacent forests, which are divided into fifty sections. Every year one section is cut and then replanted with young trees. The trees are hewn into planks in the forest and cut into slivers in the factory. The boxes are made of the outside of the trees. The factories are on the banks of lakes which are connected with one another by wide canals. Millions of matches are turned out each day. The *Scientific American* says some idea of where they all go to may be obtained from the statement that there are at least 280,000,000 of matches burned each day in the United States, or an average of five matches for each person.

Dr. Andrew Wilson's London Lectures.—The last two lectures of Dr. Wilson's course on "Physiology and Anatomy" were given in Princes' Hall, Piccadilly, on the evenings of Jan. 2 and 5. On the former date Dr. Wilson gave a lucid description of "The Heart and its Work," and dealt with the subject of "Brain and Nerves" in the last lecture. The hall was well filled on both occasions; and the beauty of the lime-light illustrations used was the theme of universal comment. The manner in which Dr. Wilson treats his subjects is eminently calculated to awaken a lively interest in matters of high importance to the nation at large. Popularisation of the teachings of physiology, and the intelligent conception of how we live and move, of what foods are necessary and which injurious, how breathing is carried out, what a brain is, and how nerves act, are items that are all too sparsely represented in the education of young and old alike. Dr. Wilson's lectures are eminently educative, and it is in this spirit, and for the good they are calculated to effect as educative means, that we are pleased to be able to accord to them our highest praise.

A CHRISTMAS CAR[R]OL[L] ON ENDOWMENT OF RESEARCH.

Few circumstances have caused true lovers of science in recent times more pain than that outcry for the Endowment of Research, to which, indeed, some few really disinterested workers and thinkers have been persuaded to lend their voices, but which has derived its volume almost wholly from those who only value science as a means of getting well-paid offices or high-sounding titles. It is an open secret that this unseemly outcry has worked much evil. It marred the harmony which had once prevailed in the Royal Astronomical Society until now the Society is to all intents and purposes silent as to science, though still occasionally noisy in debate over sinecures. Of the three who broke the peace of that Society, one was well-meaning though mistaken, one is dead, but the third still, though defeated, raises unremitting cry for gold from the sun ("Sol gold is," says Chaucer, prophetically, "and Luna silver we threpe") with the vain promise that, by sun-watching with unsnuffed candle, he will save our colonies from famine, our sailors from storm, and our farmers from foul weather.

Of all sources from which aid was hoped for by the opponents of the Endowment of Research (or rather of the would-be plunderers of the public purse who make promise of research as their plea), none could have been deemed less probable, perhaps, than the pen of the author of those cleverest of child-stories, "Alice in Wonderland" and "Through the Looking Glass." Yet, in the following lines, Mr. Louis Carroll makes fierce and effective onslaught on the advocates of suitably distributed State aid for the Endowment of Research, and on the "method of mutual admiration" by which they have striven to advance their cause—that is themselves:—

Fame's Penny Trumpet.

Blow, blow your trumpets till they crack,
Ye little men of little souls!
And bid them huddle at your back—
Gold-sucking leeches, shoals on shoals!

Fill all the air with hungry wails—
"Reward us, ere we think or write:
Without your Gold mere Knowledge fails
To sate the swinish appetite!"

And, where great Plato paced serene,
Or Newton paused with wistful eye,
Rush to the chase with hoofs unclean
And Babel-clamour of the sty!

Be yours the pay: be theirs the praise:
We will not rob them of their due,
Nor vex the ghosts of other days
By naming them along with you.

* * * *

Go, throng each other's drawing-rooms,
Ye idols of a petty clique:
Strut your brief hour in borrowed plumes,
And make your penny trumpets squeak:

Deck your dull talk with pilfered shreds
Of learning from a nobler time,
And oil each other's little heads
With mutual Flattery's golden slime:

And when the topmost height ye gain,
And stand in Glory's ether clear,
And grasp the prize of all your pain—
So many hundred pounds a year—

Then let Fame's banner be unfurled!
Sing Pæans for a victory won!
Ye tapers, that would light the world
And cast a shadow on the Sun.

"Let Knowledge grow from more to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the EDITOR OF KNOWLEDGE; *all Business communications to the* PUBLISHERS, *at the Office, 74, Great Queen-street, W.C.* IF THIS IS NOT ATTENDED TO DELAYS ARISE FOR WHICH THE EDITOR IS NOT RESPONSIBLE.

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PERPETUAL MOTION ON THE LARGE SCALE.

[1081]—In a recent number of KNOWLEDGE, p. 371-2, Mr. Proctor accuses me of assuming that perpetual motion prevails throughout the universe, and evidently regards such assumption as a grave delinquency; "really but a magnified mistake" of the same kind as that of the inventors who strive to create motion by means of mechanism.

I plead guilty of the assumption, and beg to state most distinctly that all the cosmical speculations that I perpetrated in the work referred to are based upon it. So fully is this assumption incorporated with all my conceptions of physical existence, that I cannot reason upon any physical problem, big or little, without it.

To those who are capable of being frightened by a phrase that has become a bugbear, this declaration may seem Quixotic, but if they will look a little deeper they may see that it is simply an expression of my conviction of the unassailable soundness of the broadest and grandest generalisation of modern science; and that if I err I do so in company with the greatest philosophers of this generation.

The word chosen by Mr. Proctor to express my fundamental idea is not a good one, but for that he is responsible. I have never used it; I have too much respect for her Majesty's English to speak of the "perpetuality" of motion, though the terms I prefer—viz., the "indestructibility," or "constancy," of motion distinctly imply it.

When Mr. Proctor can show me how motion can be destroyed, or ever has been destroyed; how the sum total of the motion existing in the universe can be increased or diminished by the equivalent to a falling feather, I will admit that all my conceptions of material existence are at fault, and all my philosophy is fiction.

That which cannot be destroyed, nor increased, nor diminished, must exist perpetually. W. MATTIEU WILLIAMS.

[The above letter should have appeared last week, but was unfortunately overlooked—having arrived too late for previous week—during pressure of Christmas work.

Mr. Williams rather oddly confuses error and offence. To point out a mistake is not to accuse of crime; and though Mr. Williams seems unaware of the possibility, he *may* occasionally err (without delinquency) like the rest of us. If any delinquency was suggested in my remarks on his mainly sound note in the *Gentleman's Magazine*, it was his attacking me (in what is sometimes held to be feminine fashion) without naming me, and without assuring himself that I had said what he attacked. For though I, jestingly, assumed he *must* mean somebody else, neither I nor any one else (if I can judge from letters sent me) mistook his real intent. I should have preferred a more candid and perhaps more careful criticism,—not caring much if it had been therefore more effective.

I am no more responsible for the words "perpetual motion" than Mr. Williams is responsible for the ridiculous expression "her Majesty's English." Nor does it greatly matter what name we give to the mistake by which Mr. Williams gets an infinite store of energy out of the sun's finite supply. Appealing to the constancy of the sum total of motion in the universe is as much to the purpose in the case of Mr. Williams's large perpetual machine as it would have been in the case of the small one rejected by Stephenson. In this matter Mr. Williams is no more in company with the greatest philosophers of this generation than he is in company with Newton in his theory of the generation of the solar system.—R. P.]

COLOUR OF THE SUN.

[1082]—I have looked at the sun every morning during the last few weeks through a small telescope without a sun-glass. On withdrawing my eye from the glass, everything I looked at had a yellow colour tending to orange, exactly similar to that seen when an indigo surface is looked at for some moments and the eye immediately allowed to rest on a white surface. A good deal has been written lately regarding the colour of the sun as observed in tropical regions. Would this yellow colour be complementary to that of our luminary? ALDEBARAN.

EXAMINATION OF AFTERGLOW.

[1083]—If any of the readers of your valuable paper have examined spectroscopically the brilliant afterglow which has been very marked lately, it would be of great interest if such observations were published, as they may tend to a satisfactory explanation of the phenomena. I have observed it through a small rain-band spectroscope, made by Browning, and was surprised at the very brilliant spectrum it produced, in which the whole of the *dark red* end was absorbed, while the other portions were particularly brilliant. In addition to this, I observed a wide black band in the centre of the visible red portion, and another about half as wide again just at the junction of the green and yellow portions. With this small instrument, I could not, of course, obtain any very definite results; but with a large one, with means of measuring the position, &c., of the bands, I believe some very interesting results may be chronicled, and I hope that if any have been obtained by observers, they will be published in your columns for the benefit of your readers who are somewhat puzzled at the late peculiar phenomena. T. H. DAVIS.

Peel, Isle of Man.

SUNSET GLOW.—SCIENCE OF THE "DAY."

[1084]—If the matter is of any interest, I would say that I watched the sunset here a few days ago, after the first snow-fall, but nothing unusual was seen. There was the ordinary twilight glow, but that was all. A casual observer would never have noticed any difference between that and the regular sunsets. Indeed, although I went home early to watch the sunset myself, I could see nothing unusual. The weather has been cloudy, with snow and rain ever since, so that no further observations could be made.

What do you think of the enclosed sample of "newspaper science"? The editor of this paper (*The Day*) has recently come in our midst, and has been attempting to teach us benighted people some new things. Among other matters he has been furnishing the public with some valuable (?) astronomical news, such as "The Comet was seen last night," "The Comet was again seen," &c. (You see it was necessary to let the public know that the Comet had not yet escaped). About two weeks ago (when the moon was nearly full), he startled us poor amateurs with the information, "The Comet was seen last night with the naked eye, and is moving steadily west." (!)

I rubbed my eyes, read it again, and asked myself the question, Can it be possible that an American newspaper editor has been the first to see the comet with the naked eye, and that, too, in full moonlight? Or, has the comet (having become alarmed at the publicity given to it by the Editor) turned tail, and gone towards the West, in its frantic efforts to escape? After thinking upon the matter, however, I concluded that he probably belonged to the advanced school of "Earth-flatteners," and having watched it with the naked eye (!), and ignoring the "damnable doctrine" of a round and rotating earth, had naturally concluded that it was moving in that direction.

He also "tackles" the sun spots, of which the enclosed is a sample.* I think he would be a valuable acquisition to the "Sun Spot Brigade" at Kensington. You ought to get him over there.

Baltimore, U.S.A., Dec. 24, 1883. W. H. NUMSEN.

[* The extract referred to runs thus:—"The *Day* of Saturday, in announcing that an unusually large number of spots could be seen on the sun, added that they were of the kind which usually accompany auroral displays and magnetic storms. The severe storm which prevailed all over the country yesterday shows the correctness of our deduction."]

THE GREEN SUN.

[1085]—Hartwig* relates that in 1831 a dry fog extended over a great part of the northern hemisphere, and gave sufficient light to enable small print to be read at midnight in August of that year.

* "The Aërial World."

During this fog the sun was observed to give light of a blue or green colour in the south of Europe and America.

It would be interesting to know whether such a fog prevailed over the Indian Ocean, or those parts of it from which the sun was recently seen of a green or blue colour.

The apparent connection between these two phenomena leads to the question, What is a dry fog? is it similar to the haze which is so characteristic of the east wind? (of which I have never seen any satisfactory explanation,) or is it due to some totally different cause? E. C. R.

[Dust-laden air is one form of dry fog].

THE LARGE METEOR.

[1086]—I think I saw the same meteor which "Excelsior" saw on Nov. 28 (see KNOWLEDGE, Dec. 14, p. 336). I chanced to be looking at Jupiter at 10.40, when I saw it come sailing up from the south, and it passed between me and Jupiter, and entered a very thin cloud near Ursa Major and disappeared. It was the finest meteor I ever saw; it was like a bar of red hot iron about 6 ft. long, with an intensely white-heated head, with purple colour where the body or tail commenced; and the body looked as thick as the head until it tapered off at the tail or end.

I thought the body or tail was only an optical illusion, caused by its rapid flight through the air. It was very low down, apparently not higher than two or three hundred yards, and its course was parallel to the ground. It did not look so large as "Excelsior" saw it, nor were there any sparks; perhaps it would increase much in apparent size and develop sparks in the twenty to twenty-five miles it would have to go before he saw it.

S. S. (Manchester).

STRANGE PHENOMENON (1068).

[1087]—Your correspondent "Lee Fore Brace," who sees "so many meteorological phenomena in your excellent paper," should have signed himself "The Modern Ezekiel," for his vision of wheels is quite as wonderful as the prophet's. These "wheels," which were 600 yards in diameter, would have been 2,000 yards in circumference, and having sixteen spokes (supposing them to have been equal distances apart) each spoke would have been separated by a space of 125 yards at the circumference, and, although the spokes resembled "the birch rods of the dames' schools," (by which, I suppose, we are to understand they spread out considerably) "the spokes could be distinctly seen all the way round," notwithstanding the fact that "each wheel made the revolution in about twelve seconds," which would give a velocity at the circumference of the wheel of 166⅔ yards *per second*. From the *nom de plume* he assumes it might be inferred that your correspondent is in the habit of "sailing close to the wind." This time I fancy he has "shivered his luff," and if he would avoid a "lee shore" he had better "go about." If I might be permitted to suggest a possible explanation of this wonderful phenomenon, I should say that between the time the "sun was over the fore-yard" and 11·30 p.m. there had been numerous accidents to the "*main brace*," and it had required "splicing" so often that a ray of light from a cabin window or elsewhere falling upon the water in the darkness assumed the appearance of having a rotary motion, hence the wheels. The "birch rods" would easily suggest themselves under the circumstances. A. McD.

STRANGE COINCIDENCE.

[1088]—One of my trawling vessels, the *Fairy*, of Lowestoft, while sailing through the gatway on leaving that port, on Dec. 11, by some unaccountable means lost her small boat, which was being towed astern. It turned bottom upwards, and broke the tow-rope. It being impossible in such a dangerous place to recover the boat, the vessel was obliged to return to harbour, in order to obtain another one. Having done so, the vessel proceeded on her voyage the same day.

At midnight that terrible storm burst upon them which caused such wholesale destruction amongst the fishing-vessels on the east coast.

Nearly every vessel that was fishing at the time lost the whole of their fishing gear, but my vessel, owing to the delay caused by the loss of the boat, had barely arrived at the fishing-ground when the storm came on, and thus saved her gear. After the storm, the *Fairy* proceeded on her voyage, and remained at sea for eight days' fishing, about 120 miles from Lowestoft. The master then shaped his course homewards, and had proceeded about forty miles, when, about 5.30 p.m., the crew felt their vessel strike something, and, on looking into the water, saw their own boat which they had lost eight days previously, Lowestoft then bearing W.N.W, distant eighty miles, showing that the tide had not in the least affected the direction in which the boat had drifted, but had drifted in exactly the same direction as the wind during the gale—*i.e.*, W.N.W. The boat had taken no harm, and was towed safely into Lowestoft harbour. The points that strike me as remarkable in this incident are:—1. That a boat sunk level with the water, and having only its keel exposed to the pressure of the wind, should have drifted so far in so short a time. 2. That the same vessel that lost the boat should strike it in the dark, when only a couple of yards on either side would have taken the vessel clear of it. J. W. H.

QUESTION IN ODDS.

[1089]—The following question in "Probabilities" came under my notice a short time ago. If of sufficient interest, I should be glad to see it inserted in your valuable paper; or, otherwise, a solution of it.

The odds against a certain horse winning a race are 2 to 1. Against another horse 40 to 1. What should the betting be against the two horses "coupled"—*i.e.* that neither will win?

JNO. A. JONES.

[Chance that former horse will win, $\frac{1}{3}$ or $\frac{41}{123}$; that second will win, $\frac{1}{41}$ or $\frac{3}{123}$. Chance that one or other will win is the sum these, or $\frac{44}{123}$; odds against the pair, 79 to 44.]

CYCLING.

[1090]—A correspondent in Edinburgh informs me that he considers my articles in KNOWLEDGE on tricycles are misleading from my not having been a bicycle rider, and proceeds to show me that it is not possible to pedal in the most effective manner on a "Humber" tricycle without you have learned to steer with your feet, without using your hands on a bicycle.

I wish to inform him, then, that I rode a bicycle round my dining-room table nearly twenty years ago; that I was constantly in the habit of riding with my hands in my pockets at that time, and even side-saddle, as a lady sits a horse, and that I often now ride my "Humber" tricycle without touching the handles, so that all the arguments in his long letter fall to the ground.

I have not omitted to notice that two correspondents have asked for information from me respecting the "Omnicycle." I have never actually had a day's ride on the "Omnicycle," but I have been three times on the road on machines of this make, and the more I know of them the less I like them. They are much heavier than any tricycle need be, they are lever machines, they are complicated, and, therefore, liable to get out of order, and it is difficult to alter the speed-gear while riding, though I know it may be done with practice.

No single tricycle which weighs about 100 lb. is likely to find much favour with JOHN BROWNING.

BICYCLES IN WINTER.

[1091]—Mr. Browning is mistaken when he thinks that bicycles slip upon ice and hardened snow in winter. Ice offers one of the best grips to rubber tyres that I know of; they never slip on it, strange as it may seem. That is the experience of many winter riders besides myself.

Mr. Browning recommends large tyres for winter riding. That would be all very well if the roads were constantly frozen, but how long do they remain so? My experience is that the smaller the tyres the better—they cut through the mud with an ease that must be felt to be appreciated.

Mr. Browning recommends low gearing for tricycles during the winter. From that I presume that he, after all, believes in large wheels for winter riding, and small ones geared up for summer. Is that so? I have a fancy that way also.

A large-wheeled machine is very steady on frost-bound roads among small ruts. S. RICE.

LETTERS RECEIVED AND SHORT ANSWERS.

A. McD. Thanks; your quaint letter inserted.—J. MARSHALL, G. R. There are several ways in which compensating pendulums of that kind are formed; see Sir E. Beckett's "Treatise on Clocks."—J. W. STAINFORTH—J. W. G. Only choice is between "cros" and "co"; but nearly every one says micros'copist.—DR. H. RICHES. Editor regrets, but unable.—C. W. F. states that distilled water can be obtained from the Salutaris Water Company, The Distillery, 236, Fulham-road, S.W.—NIGEL DOBLE. Are there cavernous openings, in a pile of blotting-paper, *par exemple?*—P. BICKNELL. Given at close of Editor's "Universe of Stars" (Longmans).—M. B. C. You have misapprehended both the Editor and Dr. Bali. The former never expressed objections to transcendental methods; the latter never did more than use formulæ which *would* have a meaning in space relations, if there were a fourth dimension in space. That is not an uncommon experience; but it is a very different thing from making use of a fourth dimension.

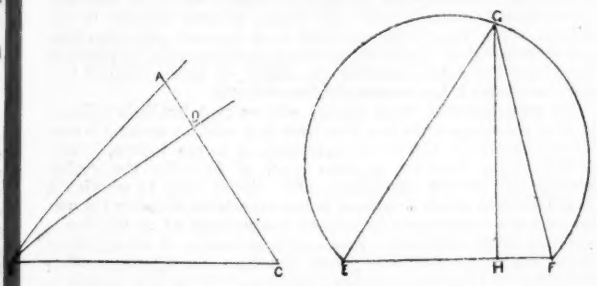
Our Mathematical Column.*

HINTS ON THE SOLUTION OF GEOMETRICAL PROBLEMS.

BY RICHARD A. PROCTOR.

(*Concluded.*)

AT present there remains only one class of deductions to deal with,—viz., those in which questions of *shape* are involved. There are many problems, which although sufficiently simple and easy, do not admit of being solved without a reference to the sixth book of Euclid; and there are others which are much more readily solved by means of the sixth book than without its aid.



Consider, for instance, the following example :—

Ex. 24.—A B C (Fig. 34, Plate I.) *is a given angle, it is required to draw a line* B D *so that when through any point* D *in* B D *a line* A D C *is drawn at right angles to* B D, D C *shall be equal to three times* A D.

Here is a problem clearly depending on *shape*—for instance, not on the *length* of B D or A C. We see that if we can divide any angle equal to A B C in the required manner our problem is solved; or rather, we have to construct a figure resembling A B C D as A B C D is *supposed* to be drawn.

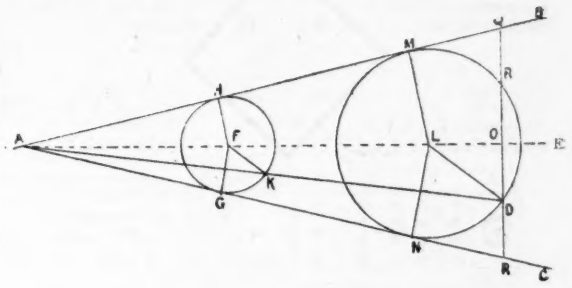
We notice that A D is one-fourth of A C and D B at right angles to A C. We therefore draw a straight line E F take H F equal to one-fourth of E F and draw H G at right angles to E F. All that is now required is that we should determine G so that the angle E G F may be equal to the angle A B C. This is readily effected since we know how to describe on E F an arc E G F containing an angle equal to the angle A B C (Euc. III. 33); the intersection of the line H G with the arc E G F gives us the required point G. We join E G, G F, then the angle E G F is equal to the angle A B C.

Now if we draw B D so that the angle A B D is equal to the angle H G F, then the remaining angle D B C is equal to the remaining angle H G E. Through any point D in the line B D thus obtained draw A D C at right angles to B D. Then obviously the triangle A B D is similar to the triangle G H F, therefore A D is to B D as F H to H G; similarly B D is to D C as G H to H E; therefore *ex æquali* A D is to D C as H F to H E. But H F is one fourth of E F; therefore H E is equal to three times H F; and hence D C is equal to three times A D.—Q.E.F.

We do not give the considerations which lead to the last lines of the proof. The considerations respecting shape which led to the construction may be looked on as obvious, although (as is often the case) it may not be quite so obvious how the *proof* is to be made to depend on properties established in the sixth book.

In a problem of the above type we cannot well avoid the use of the sixth book. I now give a problem which can be solved by the third book, but one can scarcely doubt that the solution depending on the sixth book is that which would *naturally* occur to a person dealing with the problem as a new one :—

Ex. 35.—*Let* A B, A C (Fig. 35, Plate I.) *be two straight lines meeting in* A, D *a given point. It is required to draw a circle which shall pass through the point* D *and touch the lines* A B, A C.



We first notice that the circle must have its centre in the line A E which bisects the angle B A C. For taking any point F on this bisector and drawing perpendiculars F H and F G on A B and A C respectively, we see that F H is equal to F G (since the triangles F A H, F A G are equal in all respects, Euc. I., 26). We describe a circle H G K with centre F and distance F H or F G, and touching A B and A C in H and G, Euc. III., 16. But this circle does not pass through D. It is obvious, however, that if we draw A K D, cutting the circle H G K in K, then the figure formed by the lines A H, A G, the point K, and the circle H G K exactly *resembles* that which will be formed when our problem is solved. If then we can only form a figure resembling that we have constructed but such that the circle shall pass through D, the problem will be solved. Now we see that F lies in a defined direction with respect to K,—in other words the angle A K F does not vary with the size of the circle drawn as H G K was drawn. We have then only to draw D L so that the angle A D L is equal to the angle A K F—that is, we have only to draw D L parallel to K F, in order to determine L the centre of the circle we require. The proof runs thus :—

Draw L M and L N perpendicular to A B and A C, respectively. Then, from the similar triangles A L D, A F K, L D is to A L as F K to A F. Again, from the similar triangles, A L N, A F G, A L is to L N as A F to F G. Therefore *ex æquali* L D is to L N as F K to F G. But F K is equal to F G; therefore L D is equal to L N—that is to L M. Hence a circle described with centre L at distance L D will pass through M and N, and touch A B and A C in these points, respectively, since the angles at M and N are right angles.

Note.—Of course there is no difficulty in solving this problem without the aid of any book beyond the 3rd. The obvious course of proceeding is by way of analysis. Let D N M be the required circle. A L E is the bisector of the angle B A C and can be drawn at once. R D O P Q at right angles to A E can also be drawn, and gives P a point on the required circle for D O = O P. Then one can hardly miss the relation that square on R N is equal to the rectangle under R D, R P; whence N is given, and the required circle, passing through the three known points, P, D, N, is determined.—Q. E. F.

DISTILLED WATERS.—It may interest readers of Mr. Dawson's article on the means of prolonging life, to know that for some years the Messrs. Packham & Co. have been engaged in the work of introducing pure waters, made exclusively from distilled water, and have established at Croydon a model factory for their production. Dr. B. W. Richardson, who took the chair at a meeting of the Society of Arts on Dec. 5, on the occasion of a paper being read by Mr. T. P. Bruce Warren, in which the use of distilled water for the manufacture of artificial mineral waters was strongly advocated, paid Messrs. Packham a visit a week or two since, and critically examined the whole of the processes, with which he expressed much satisfaction.

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* This paper concludes the Hints. It has been decided, in response to a great number of communications, to follow with a series of geometrical papers on the several books of Euclid, accompanied by very easy illustrative problems. It had been proposed to introduce between the Hints and these (long since prepared) papers, two series of papers of great interest on special methods: but so many readers have asked for papers on the more familiar subjects that the sub-editor has felt it but right to consider their wishes. As Mr. Proctor's papers already written extend to the middle of the third book, while the collection of illustrative problems runs as far as Prop. 34, Book I., there is no fear that the supply will run short before the Editor's two months' holiday is finished. Next week however a note on the old problem of Achilles and the Tortoise which the sub-editor found in Mr. Proctor's portfolio will appear.

Our Whist Column.

By "Five of Clubs."

THE following game, from the *Westminster Papers*, illustrates the disadvantage often arising from leading trumps at the score Four all.

The Hands.

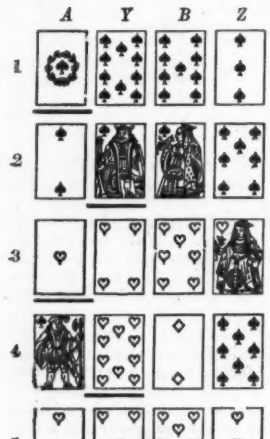
Y.
Hearts—Kn, 10, 6, 5, 4.
Clubs—Q, 6.
Diamonds—A, K, 6, 4.
Spades—K, 10.

B.
Hearts—K, 9, 8, 7.
Clubs—Kn, 9, 5, 4.
Diamonds—Q, 5, 2.
Spades—Q, 9.

Y B
Score—
Four all.
Dealer.
A Z
Trump
H. Q.

A.
Hearts—A, 2.
Clubs—2.
Diamonds—Kn, 8, 7, 3.
Spades—A, Kn, 6, 5, 4, 2.

Z.
Hearts—Q, 3.
Clubs—A, K, 10, 8, 7, 3.
Diamonds—10, 9.
Spades—8, 7, 3.



Note.—The card underlined wins the trick, and card below leads next round.

Notes and Inferences.

1 and 2. *Y* and *B* hold no more Spades, and *A* holds the three lowest at least.

3. Leading trumps when the score is "Four all" is not generally advisable. In the present case *Y* should have led Diamonds—King, then Ace, then a little one; this his partner would have ruffed, leading King and Ace of Clubs, and then a little one, which *Y* would have ruffed (unless *A* played his trump Ace); then another Diamond lead would have forced the game, whether *B* played his trump King or not. This was the right line of play at the score.

4. *A* (the Editor of the *Westminster Papers*) rightly forces *Y*, and *B* plays well in passing the trick. The command in trumps thus reverts to him.

5. *B* safely plays the Eight, the Knave being certainly with *Y*.

7. *Z* sees that the tenace in Clubs remains with *B*; and apart from the discard at trick 4 he knows that *B* is weak in Diamonds. *Y*'s play at trick 4 shifted the trump strength, and *B* properly therefore discarded from his weakest suit in playing to that trick.

8. As the cards actually lie, a Club lead here would have won the game, or rather have shown *Y* an easy course to win. But *Z*, our skilful correspondent Mr. Lewis, would have played very ill—and, therefore, very unlike himself—had he led a Club here. He sees that the only chance left is that *Y* may be strong in Diamonds, but he has no just reason for supposing *Y* quite so strong as he is. The proper course is to lead through *A*, *B* being certainly weak in Diamonds. Thus *Y* gets the best chance of making two tricks in Diamonds, and winning.

But (8, 9, and 10) *Y* makes his two tricks in Diamonds, and has the game in his hands, yet throws it away. He knows certainly from *Z*'s play at trick 9 that the winning Diamonds are with *A*, and should have been able to form a shrewd guess that *both B*'s Clubs cannot be winning ones, or *Z* would not have played as he did. At any rate, the only chance of saving and winning the game lay here. Leading a Diamond at trick 10 was handing the game over to the enemy.

Our Chess Column.

By Mephisto.

OPENINGS.

EXPERIENCE has proved that there is a defence to every attack, and that, therefore, an opening is only dangerous as long as it is imperfectly understood. The complicated and ingenious attacks of the Muzio and Algaier Gambits were in former times considered very formidable, and it was dangerous to meet these openings. Until within comparatively recent years the Evans Gambit attack proved stronger than the defence thereto. The combined skill of many thousands of even average players, represented by a corresponding number of games, has proved itself stronger than any of these attacks. With the increase of Chess columns to register the results of ordinary play—as in one game out of many chance or skill is certain to discover some new mode of meeting an attack—the strength of former favourite openings was gradually reduced until the attack became inferior to the defence. The Ruy Lopez is based upon sounder principles than any other opening. Analysis, therefore, as represented by practical play, has nevertheless modified the attack, as in the case of less sound openings it has completely demolished it.

We must, however, warn weaker players that but little reliance is to be placed upon the fact that such and such an opening is considered unsound. Unless the principles of it are known, it will, curious to say, have the opposite result of defeating the weaker player with greater expediency. We cannot omit to mention a comic incident which happened in our experience, showing the uselessness of relying upon the declared unsoundness of an opening to defeat a strong adversary. Scene, a Chess resort. A strong player and a Yankee contest some games together. Strong player offers the Algaier Gambit. Yankee exclaims, "Why, this attack is obsolete. You cannot play tricks upon me." Strong player continues unabashed, and defeats his mortified adversary.

Seeing, then, that the greatest danger proceeds from the ignorance of openings, we will endeavour to make our readers sufficiently familiar with the leading variation of every opening, to enable them to reach the middle of the game without falling a prey to the superior knowledge of an antagonist.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess Editor.*

Henry Bristow.—Your question in reference to No. 33 probably refers to the Black QBP. There are two grounds for making P takes P en passant impossible. Firstly, a P can only he taken en passant immediately on its being moved. Secondly, the P might have moved from Q3 to Q4, and not from Q2 to Q4, in which case it cannot be taken en passant at all.

S. Harvey.—Problems received with thanks.

S. B. B.—You probably meant R to B sq instead of B8.

W. S. Brindle, D. Matheson (not impossible), Richard Maurice.—If 1. R takes B, Black has a clever defence in 1. R takes BP, upon which there is no mate next move.

Alfred W. Brandon.—Solution incorrect.

E. R.—You can obtain diagrams of printers, 8, Salisbury-court, Fleet-street, E.C.

Correct solutions received.—Problem No. 109, Henry Bristow. No. 110, R. Willis, A. Conrad Smart, W. A. Bartels, Notnab, E. R., Kingsbridge, John, M. T. Hooton, W., H. A. N., A. Schmucke (thanks), Alex. S. Orr, George Johnson, Berrow, Rev. W. Anderson. Jas. Wade, London.

J. B.—In the first diagram Blackburne *v.* Appleton, p. 398, the Q got accidentally shifted from Kt3 to B3. After 34. Q to Kt8(ch), K to R2. 35. R to K8, P to Kt3. 36. P takes P, P takes P. You will see that, if the K takes P instead, 37. Q to Kt3 (ch) would win. (Thanks.)

Tom.—Problems received with thanks.

J. Dunlop.—You will find the problem quite right. Have forwarded your note. On the first move alone there may be 400 different positions in a game, each party having twenty first moves.

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